DOCUMENT RESUME

ED 202 058 CS 503 360

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TITLE Prospects for Significant Theoretical Advances in Communication: The Role of the Interesting Question.
PUB DATE Apr 81
NOTE 20p.; Paper presented at the Annual Meeting of the Central States Speech Association (Chicago, IL, April 9-11, 1981).

EDRS PRICE MF01/PC01 Plus Postage.
DESCRIPTORS *Communication (Thought Transfer); *Communication Research; *Inquiry; *Questioning Techniques; *Research Methodology; Research Needs; *Speech Communication

ABSTRACT

This paper discusses ways in which the field of speech communication can be advanced. The first half of the paper characterizes the objectivist and subjectivist views of how knowledge is acquired and the forms of inquiry to which these views have led. The remainder of the paper demonstrates the role that the "interesting question" (one for which the answer promises to have significant impact on understanding how particular sets of phenomena are related) has played in contributing to advances in knowledge. Some cases are examined in which the role of the interesting question has led to important and useful discoveries. The cases examined include (1) John Snow's solution to the cholera problem in nineteenth century England, (2) the discovery of penicillin, (3) the development of the thoery of atomic structure in chemistry, (4) Claude Bernard's discovery that the body produces its own sugar, and (5) Charles Darwin's work on evolution. Based on these observations, the conclusion is drawn that speech communication scholars should continue to direct their energies toward discovery rather than justification. (RL)

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PROSPECTS FOR SIGNIFICANT THEORETICAL ADVANCES IN COMMUNICATION: THE ROLE OF THE INTERESTING QUESTION

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Central States Speech Association Convention

Chicago, Illinois

April 11, 1981

CS503360

PROSPECTS FOR SIGNIFICANT THEORETICAL ADVANCES IN COMMUNICATION: THE ROLE OF THE INTERESTING QUESTION

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For many years, if not for the duration of its existence, the field of Speech Communication has been concerned with the matter of how best to advance the knowledge in which its representatives are ostensibly interested. Whereas much of the concern about the question has focused on identifying concepts that would give the discipline distinctiveness, theoretical positions that are most promising, and methodological perspectives that are most defensible for conducting research, more recent pursuit of the issue seems to entail an essentially epistemological flavor. In other words, debate has become increasingly centered on the fundamental question of how we know, or, perhaps more accurately, which view of how we know is the one to which scholars ought to subscribe.

To some extent, the issue appears to be misplaced. In other respects, however, it has significance for how the field will develop. My own view is that although the question is important, it may be that we are becoming so occupied with epistemological matters that we will lose sight of what it is that we want to know. The real source of progress in any discipline is not essentially epistemological. Rather, as I contend and shall try to demonstrate, it is the interesting question. Before I can develop this thesis, however, it seems necessary to explore the implications of the espistemological dispute for the advance of inquiry into communication phenomena. This exploration, I would hope, will reveal more clearly the rationale for my denial of the proposition that the

source of knowledge is epistemological and the feeling that the question of how we know has been overemphasized.

To acknowledge the disagreement in responses to the question of how we know is more or less to divide people concerned into two classes: those who believe in a discoverable objective reality and others who deny the possibility. Reality, the latter group appears to believe, is subjectively experienced and/or created. Hence, it is unknowable in the sense that an objectivist position suggests. As one examines the arguments in support of each position, his or her impulse is to keep shifting allegiances because both make sense at some levels of thought and little sense at others. Both schools, moreover, at times appear to make contradictory claims and are forced to retreat almost paradoxically to one another's premises to find validity in their own.

Having made these observations, let me be somewhat more specific in characterizing the objectivist and subjectivist view of how we know. I shall begin with the objectivist position. The presumption of this perspective on knowing is perhaps best articulated in the following remarks by C. I. Lewis:

> Unless the content of knowledge is recognized to have a condition independent of the mind, the peculiar significance of knowledge is likely to be lost. For the purpose of knowledge is to be true to something which is beyond it. Its intent is to be governed and dictated to in certain respects. It is a real act with a real purpose because it seeks something it knows it may miss. If knowledge had no condition independent of the knowing act, would this be so?[1]

Stating a presumption, unfortunately, is not a demonstration of how the ideal it embodies can be realized. Peirce has spoken more directly to the issue in stating that, "Different minds may set out with the most antagonistic views, but the progress of investigation carries them by a force outside of themselves to one and the same conclusion." Moreover, "the opinion which is fated to be

ultimately agreed to by all who investigate is what we mean by the truth, and the object represented in the opinion is the real."[2]

It is interesting that Peirce, who is clearly identified with objectivist thinking, espoused a description of reality that is remarkably similar to Habermas' consensus theory of truth; that is the notion that truth is "the ideal permanent consensus of scientists at the limit of their method of testing and self-correction" and that reality is "the totality of possible true statements."[3] In Peirce's view, however, such statements still correspond to something "out there." As he puts it, ". . . it is necessary that a method should be found by which our beliefs may be determined by nothing human but by some external permanency . . . . There are real things, whose characters are entirely independent of our opinions about them."[4]

In spite of the intuitive appeal that the objectivist position, as portrayed by Peirce and others of his leaning, has, the problem of knowing when one has discovered the truth and what is real remains unsolved and seemingly a logical impossibility.[5] Invoking such notions as "successive approximations of the truth," equating empiricism with knowledge, or developing the essentials of the so-called scientific method, moreover, do little to create grounds for believing that the problem will ever be solved. The first tactic seems to beg the question and the latter two to avoid it.

In the final analysis, perhaps the best that can be said of the objectivist view of knowing and the forms of inquiry to which it has led is that it has contributed to our ability to make better predictions about many aspects of our physical and social environment. This is no small achievement. To the extent that we value such accomplishments, the objectivist contribution has been meaningful, its failure to live up to the ideal notwithstanding. As Scheffler has observed, "Science, generally, prospers not through seeking impossible guarantees, but through striving to systematize credibly a continuously expanding experience."[6]

The limitations of the objectivist assumptions concerning the nature of reality and the discoverability of truth, on first thought, would seem to favor the subjectivist position. Indeed, the burgeoning of essays on phenomenological approaches to the study of communication as well as other subjects and the continual sounding of the death knell for empiricist and positivist thinking by subjectivist critics conveys precisely such an impression. From the point of view of those who missed their funeral or who, in Twain's words, regard the announcement of their death as "greatly exaggerated," however, proponents of the subjectivist view would do well to examine the logical foundations of their own conception of reality and truth.

If one subscribes to the notion that reality is merely a social construction and truth its subjective manifestation, it seems as if he or she is paradoxically making an objective claim. Are we to presume, therefore, that the subjectivist position admits of one bit of objective, knowable reality? If so, then its conception of reality and truth is inconsistent in the denial of the very premise on which its credibility rests. If there is one objective claim that we can accept by somehow knowing that it is true, then why not two, three, and so on? If the proponents of the subjectivist view do not admit of the exception described, then on what basis is there any reason to believe it--the subjectivist view, that is? It is beyond demonstration.

The problem here is analogous to holding that for every rule there is an exception. If that be the case, then presumably there is an exception to the rule that every rule has an exception, which seems to imply that what is the case cannot be the case.[7] And who among us would want to admit that he/she harbors such an inconsistent logical possibility?

Another difficulty associated with the subjectivist view--more particularly in its proponents' critique of objectivism--lies in the apparent assumption that if the indictments against objectivist thinking are warranted, the appropriate-

ness of the competing view is ipso facto established. Locating the weaknesses in one view does not automatically imply the acceptability of the other although it may admittedly enhance its credibility. The point is that the inability to devise a means of discovering objective reality does not constitute conclusive evidence of its non-existence. In this sense, the subjectivist view of reality leaves us no better off than the objectivist view.

The fact that both the objectivist and subjectivist suffer from problems of logical consistency is not a cause for great alarm. Both have been of value in promoting the kind of dialogue that provides us with a better understanding of the assumptions to which particular ways of investigating questions of interest commit us.

In addition, research generated from both perspectives is amenable to assessment in relation to what Hesse calls "the pragmatic criterion"; that is the extent to which various sorts of occurrences can be successfully predicted under a set of specified conditions.[8] The instrumental value of inquiry, when considered from this frame of reference, favors neither position.

Finally, the competing perspectives can promote a mutuality that may ultimately increase our success in meeting the pragmatic criterion. I am assuming here, of course, that part of the concern among those caught up in the epistemological dispute relates to research practices as well as to the nature of the claims to which inquiry leads. If we are concerned only about what kind of truth it is that knowledge claims represent, the kind of mutuality about which I am speaking is irrelevant. My feeling, however, is that most of those who have addressed the issue are indeed concerned with the implications that objectivist and subjectivist assumptions have for the practice of inquiry even if the concern is not always evident in the studies presumably generated from each perspective. Otherwise, I can conceive of no useful reason for people other than philosophers of science even dealing with the subject of epistemological issues.

As an example of the way in which one perspective can be useful to researchers conducting investigations from the other, consider the following hypothetical case. Assume that an investigator has reason to suspect that a specified message property has behavioral consequences for those to whom a given message is addressed. From a purely objectivist point of view, nothing more than a logically and theoretically defensible reason would be necessary to warrant the hypothesis. Now suppose that the anticipated finding when the hypothesis is subjected to rigorous experimental test does not surface. The investigator might begin to search for possible reasons consistent with general objectivist notions. He or she could surmise that the theory is in error, the independent variable was not effectively controlled, the measuring instruments were insensitive, and the like.

The investigator in the example might be correct in assuming that any or all of the explanations considered contributed to his or her failure to confirm the hypothesis. On the other hand, the difficulty might be that the stimulus property of interest, perceptually speaking, was not received or interpreted in the manner necessary for the suspected consequence to occur. This explanation is more consistent with general subjectivist thinking. Were our objectivist-minded researcher to give it serious consideration, it might lead him or her to the realization that another, perhaps better, test of the hypothesis is possible.

Reversing the coin, we can conceive of the sort of situation in which a subjectively oriented researcher generates a prediction about message effects on the basis of what communication targets believe or feel influences them only later to discover that the prediction based on such information fails. From an objectivist point of view, one might argue that it is not necessarily what people think influences them that necessarily does. I recall a graduate student of my acquaintance once telling me that his beliefs could not be conditioned because God has given us free will. The assertion strikes one as the direct result of the very process it seeks to deny. The point of this example is to suggest that

an expectation derived from a subjectivist perspective can and often probably should lead one to consider possibilities more consistent with an objectivist view because the individual's phenomenal world is not always reliably or easily accessible. This, of course, was a major factor in the move away from the introspectionist school of psychology in the nineteenth century and the subsequent development of behaviorism with its strong objectivist leanings.

I have gone on at some length discussing objectivist and subjectivist viewpoints without as yet developing my thesis. As I intimated earlier, however, my reason for doing so was to try to establish that advances in communication inquiry are not contingent on a final resolution of the objectivist/subjectivist dispute inasmuch as both have logical problems that render neither superior to the other. From the preceding discussion, I trust that the basis for the assertion is now clear. In addition, I would hope that it is clear that inquiry generated from both perspectives can be assessed in terms of its instrumental value equally well by means of the prgmatic criterion. Finally, each perspective has potentially useful and explanatory value in accounting for the failure of research generated from the other to provide confirmation of hypothesized relationships among variables in which investigators are interested.

The view I am promoting may suggest a kind of philosophical ecclecticism that epistemological purists will find abhorrent; however, I am not convinced, at least for the moment, that the interests of communication inquiry can best be served in any other way. Nor am I convinced that a preoccupation with philosophical presuppositions about ultimate questions of knowing is especially desirable. Although it is surely reasonable to expect practicing theorists and researchers to understand something of the epistemological assumptions on which their claims to knowledge rest, expecting them to achieve complete closure concerning the validity of such assumptions is not. Despite the artificiality of the boundaries and the ease with which they are violated, it is nevertheless use-

ful to bear in mind that researchers and theorists function principally in the "context of discovery" and not in the "context of justification."[9] The extent to which we need to be concerned with epistemological issues is suggested by that distinction.

However valuable it may be for one to understand the limitations that one's epistemological frame of reference imposes on inquiry and on judging what its results reveal, he or she must also remember that it is not this frame of reference which initially, or e· ultimately, is the source of advances and alterations in our understanding of the environment in which we reside. To accept such a possiblity would be to assume a logic of discovery that as far as anyone can tell does not and will never exist. To what, then, can one attribute progress in knowledge? An examination of the history of scientific achievement would seem to suggest the primacy of the interesting question.

In the remainder of this essay, I do not intend to attempt a listing of the interesting questions on which communication scholars should be focusing. First, I cannot be confident of what they are. Such questions seem to arise in conjunction with certain situational factors that may be subject to sudden and radical change. They are, moreover, frequently encountered quite by accident. Finally, simply developing a list of what I find to be interesting would have little value in supporting the thesis. What I rather seek to do, with the aid of some historical examples, is to demonstrate--albeit in limited fashion--the role that the interesting question has played in contributing to advances in knowledge. Let me first define my terms, however.

An interesting question, as I am using the expression, is one for which the answer promises to have significant impact on what we consider or understand to be the way in which particular sets of phenomena are related. Ordinarily, this would imply a fundamental alteration in commonly accepted views of particular relationships. The answers to interesting questions, of course, do not always

have such impact. The question is considered interesting, however, because it potentially has the consequence of leading to some new level of undertanding. The conception that I am trying to verbalize here may come into sharper focus if one first considers the distinction between scientific inquiry as often portrayed and as usually practiced.

Ravetz sets the tone for examining the difference between science in principle and science in practice in the following observation:

> The question 'What is Science?' supplies the title or the subject matter of many books on the 'philosophy of science'. In them, the question usually takes the form, 'What sort of truth is embodied in completed scientific knowledge?' Ideas developed in the course of an attempt to answer such a question will not be well suited for describing science as a human activity, always changing and never perfect. Treatises on 'Scientific Method' written within such a framework of ideas seem to have little relation to the real work of discovering new knowledge and are frequently scorned by practicing scientists who have become amateur philosophers of science.[10]

What scientists really do was the subject of an exhaustive examination of scientific thought on the nature of the universe from the Ionian period through the age of Isaac Newton by Arthur Koestler. On the basis of his study, Koestler has asserted that:

> The progress of Science is generally regarded as a kind of clean, rational advance along a straight ascending line; in fact it has followed a zig-zag course, at times almost more bewildering than the evolution of political thought. The history of cosmic theories, in particular, may without exaggeration be called a history of collective obsessions and controlled schizophrenias; and the manner in which some of the most important individual discoveries were arrived at reminds one more of a sleepwalker's performance than an electronic brain's.[11]

One need not be enchanted by Koestler's rhetorical excesses to appreciate his point. That is, the reality of scientific achievement is not particularly, if at all, consistent with idealized descriptions of it. There are many cases that lend credence to his thesis. What seems to be crucial to the creative process, then, is a natural curiosity and imaginative outlook that leads one to formulate and pursue interesting questions. As Koestler puts it, discovery is often a result of the "capacity of perceiving a familiar object, situation, problem, or collection of data, in a sudden new light or new context."[12]

While this view certainly does not preclude all elements of rationality from scientific inquiry, neither is it consistent with the more popular notion that scientific progress is governed by some clearly defined, well understood, and generally practiced rational process. But even if it were, the fact remains that the inquirer must still generate questions. If the questions are without the potential for creating new levels of awareness and understanding, there is very little that any method, however rationally conceived, can do to produce such an outcome. Let us now examine some of the cases in which the role of the <u>interesting question</u> so conceived has led to important and useful discoveries.

During the nineteenth century, cholera was a dread disease in urban centers throughout the world. In spite of the number of theories in evidence, the spread of the disease was not well understood. As a result, little could be done to control it during periods in which it reached epidemic proportions. Not until an English physician by the name of John Snow began to ponder the question of what factors distinguished those who contracted cholera from those who did not was an effective solution eventually forthcoming.

Such data as the fact that doctors who treated cholera victims were relatively free of the disease Snow found to be disturbing. Noting the broad range of possibilities that accounted for the differences, he was eventually able to pinpoint the water supply of London and the different sources of drinking water

to which the two populations had access as the critical variables. Greater volumes of the bacteria causing cholera were present in the water supply of those usually lower class people who inhabited the overcrowded urban center.

Since the germ theory of disease was not widely accepted, or in some cases even known, at the time, others attributed the source and spread of cholera to the personal characteristics of its victims. By asking a question rather than presuming the efficacy of existing explanations, Snow was able to advance knowledge not only in a meaningful way but one that also provided the basis for treatment of a heretofore insoluable medical problem.[13]

Another, perhaps more well known, example that we can consider is the discovery of penicillin. Usually portrayed as an instance of the role of chance in scientific research, the story is somewhat more involved. Although it is true that the stimulus for Alexander Fleming's work on penicillin was the result of a chance observation, the occurrence led to a question that others making the same observation had failed to raise. As Beveridge relates the incident,

> Fleming was working with some plate cultures of staphylococci which he had occasion to open several times, and as often happens in such circumstances, they became contaminated. He noticed that the colonies of staphylococci around one particular colony died. Many bacteriologists would not have thought this particularly remarkable for it has long been known that some bacteria interfere with the growth of others.[14]

Fleming had asked a simple but nonetheless interesting question and in so doing eventually made an important contribution to the subsequent development of antibiotics.

A third illustration of the role of the interesting question can be found in the annals of chemistry. Until late in the nineteenth century, atomic theory permitted the prediction of chemical facts only in respect to inorganic substances. The structure of organic compounds was not well understood. As Goldstein

and Goldstein point out, "A surprisingly large number of different compounds had been discovered, and in them it seemed that any numbers (sic) of different kinds of atoms could be present."[15] Not until Friedrich Kekulé raised the question of how carbon atoms might be arranged in forming molecules with other elements was the problem solved and the major advance of organic chemistry made possible.[16]

The discovery of carbon chains reportedly came to Kekulé in a dream, which perhaps qualifies it as one of the clearest and most literal examples of Koestler's sleepwalker thesis.[17] Whether the absence of Kekulé's nap would have precluded the discovery, of course, one cannot say. It seems safe to conclude, however, that had he not raised the question and been pondering it prior to the moment of insight, the challenge to the validity of atomic theory posed by organic compounds might well have remained unresolved for some time to come.

Still another example of the role of the interesting question in scientific research is reflected in the work of Claude Bernard. Before the time of Bernard, it was commonly understood among scientists that animals had to obtain carbohydrates, fats, and proteins from plants. In asking the question of how sugar is metabolized, however, Bernard happened upon the discovery that the body produces its own sugar from substances that have no sugar content.[18] Although the original question was not aimed at the discovery to which it led, the fact that Bernard pursued a question for which the answer would shed new light on body chemistry enabled him to make the kind of serendipitous finding that has even greater significance than the information initially sought.

As a final extended illustration of my thesis, I have chosen the case of Darwin's work on evolution. Prior to the publication of The Origin of the Species in 1859, the prevailing view was that the species were independently created--a conception that permitted a certain degree of compatibility with some of the dominant theological views of the day. Darwin was to have a profound effect on this relationship by radically altering conceptions of how we came to be.

How the theory of evolution developed is a subject possibly best described by Darwin himself. For this reason, I have quoted at some length from the introduction to his famous work:

> When on board H.M.S. 'Beagle,' as naturalist, I was much struck with certain facts in the distribution of organic beings inhabiting South America, and in the geological relations of the present to the past inhabitants of that continent. These facts . . . seemed to throw some light on the origin of the species--that mystery of mysteries, as it has been called by one of our greatest philosophers. On my return home, it occurred to me in 1837, that something might be made out of this question by patiently accumulating and reflecting on all sorts of facts which could possibly have any bearing on it. After five years' work I allowed myself to speculate on the subject, and drew up some short notes; these I enlarged in 1844 into a sketch of the conclusions, which then seemed to me to be possible: from that period to the present day I have steadily pursued the same object. I hope that I may be excused from entering on these personal details, as I have given them to show that I have not been hasty in coming to a decision.[19]

What is most clear from this bit of self-reflection is that a disturbing but nevertheless interesting question suggested by facts that would not square with existing notions took Darwin on an extended intellectual journey to what has since become a dominant theoretical position in the biological sciences. It further demonstrates that if a question is sufficiently interesting, it can occupy one for an extended period of time. In this particular case, the persistence appears to have been well worth displaying.

One could continue listing example after example, such as Jenner's work on the treatment of small pox, Harvey's achievements in convincing a naive medical community that blood circulates, Pasteur's contributions to the germ theory of disease, Lister's cultivation and propagation of antiseptic practices, Salk's

contribution to the development of polio vacine, and, of course, Einstein's revolutionary reconceptualization of the nature of the universe and the relationship of matter to energy. As with the preceding examples, in each of these cases the genesis of gains in our understanding was a natural but disciplined curiosity that stimulated interesting questions.

Cases of the kind I have been discussing hold some valuable lessons for the advancement of communication as a field of study. The most important of these is that we are probably better off in the long run continuing to direct our scholarly energies toward discovery rather than justification. As I mentioned at the outset, there is the very real possibility of our becoming so excessively concerned about the nature of inquiry and what is the right perspective from which to conduct it that we shall lose sight of the reasons for being involved in it in the first place. The constraining influence of such concerns, moreover, can serve only to perpetuate the sort of safe investigation of trivial problems and obvious relationships about which some critics already so vehemently complain. In other words, we may pass over interesting and potentially important questions by being too sensitive to the limitations on inquiry implicit in different perspectives on the issue of knowing. In a field not presently recognized for the boldness of its generalizations, this type of conservatism could work to our disadvantage.

Although one need not envision achievements on quite as grand a scale as those chronicled in this essay, insofar as I am capable of judging, there is nothing about communication per se that renders it less amenable to major discovery than any other subject of interest. In fact, because the phenomenon of communication is so pervasive, what we would like to know about it may be more easily accessible than relationships of interest to the representatives of other disciplines. In this sense, I would argue that the prospects for significant theoretical advances are very good.

Jerome Ravetz has observed that:

> To be involved in a field just entering maturity is the most rewarding career for a scientist; for then one can make the greatest achievements at relatively little risk. But estimating the points of transition between phases is a very difficult task; a field or area of science which is approaching senescence is a dreary place; and immature fields with the hope of imminent maturation are, with all of their attendant harzards, the place where the greatest challenge is found.[21]

That challenge, it seems to me, can only be met, however, if we seize upon the opportunities that our lack of maturity has provided to raise and pursue the <u>interesting question</u>.

Footnotes

[1] Clarence Irving Lewis, Mind and the World Order (New York: Dover Publications, Inc., 1956). Cited by Israel Scheffler, Science and Subjectivity (Indidianapolis, Indiana: Bobbs-Merrill, 1967), p. 7.

[2] Charles S. Peirce, "How to Make Our Ideas Clear," in Essays in the Philosophy of Science, ed. Vincent Thomas (Indianapolis, Indiana: Bobbs-Merrill, 1957), pp. 53-54.

[3] Mary Hesse, Revolutions and Reconstructions in the Philosophy of Science (Bloomington, Indiana: Indiana University Press, 1980), p. 209. Habermas is not one whom most historians and philsophers of science would identify as a representative of the objectivist position.

[4] Cited by Justus Buchler (ed.), Philosophical Writings of Charles Peirce (New York: Dover Publications, Inc., 1955), p. 18.

[5] Kaplan has discussed the inherent difficulties associated with a so-called logic of discovery, which presumably would be the only way in which one could hope to have certain knowledge of the truth. As he suggests, however, "To ask for a systematic procedure which guarantees the making of discoveries as a corresponding procedure guarantees the validity of a proof is surely expecting too much." See Abraham Kaplan, The Conduct of Inquiry (San Francisco: Chandler, 1964), p. 15. Salmon, speaking in a similar vein and articulating the apparent consensus of other philosophers of science, has observed that, "To suggest that there might be a mechanical method that will necessarily generate true explanatory hypotheses is a fantastic rationalist dream. Problems of discovery completely aside, there is no way of determining for certain that we have a true hypothesis." See Wesley C. Salmon, The Foundations of Scientific Inference (Pittsburgh, Pennsylvania: University of Pittsburgh Press, 1966), p. 112.

[6] Scheffler, p. 124.

[7] For an extended discussion of the logical paradox in the subjectivist view of reality, see Scheffler's essay, "Observation and Objectivity," in Science and Subjectivity, pp. 21-44.

[8] See Hesse's essay, "In Defence of Objectivity," pp. 167-86.

[9] See Kaplan, pp. 1-33, and especially 12-18, for a much more extensive discussion of the contexts of "discovery" and "justification" as they relate to the scientist's "logic in use" and the historian and philosopher's "reconstructed logic." The implicit division of labor between practitioner and critic suggests a reciprocal process that facilitates the practice of inquiry and directs it toward more useful ends.

[10] Jerome Ravetz, Scientific Knowledge and Its Social Problems (New York: Oxford University Press, 1971), p. 17.

[11] The Sleepwalkers (New York: Macmillan, 1959), p. 15.

[12] Ibid., p. 519.

[13] For an extended discussion of this case, see the account by Martin Goldstein and Inge F. Goldstein, How We Know (New York: Plenum Press, 1978), pp. 25-62.

[14] W. I. B. Beveridge, The Art of Scientific Investigation (New York: Random House, 1957), pp. 217-18.

[15] Goldstein and Goldstein, p. 193.

[16] Beveridge, p. 76.

[17] Ibid., p. 76.

[18] Ibid, pp. 216-17.

[19] Charles Darwin, Origin of the Species, the Mentor edition (New York: The New American Library of World, 1959), p. 27.

[20] See, for example, Gerald R. Miller, "On Rediscovering the Apple: Some Issues in Evaluating the Social Significance of Communication Research," Central States Speech Journal, 30 (1979), 14-24; Ernest G. Bormann, Communication Theory (New York: Holt, Rinehart and Winston, 1980), pp. 128-83.

[21] Ravetz, p. 402